

WOMEN, SCHOOLING, AND LABOR FORCE PARTICIPATION, 1900-1920:
SOME REFLECTIONS ON THE USE OF QUANTIFICATION
IN SOCIAL HISTORY

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"That Bitch-goddess, QUANTIFICATION!" cried social and intellectual historian Carl Bridenbaugh in his 1962 presidential address to the American Historical Association.¹ The memory of his exclamation rang through my ears as I read the paper, "Women, Schooling, and Labor Force Participation." But then I paused--wondering--what was it that so challenged my historical perspective when I read this paper by Professor Rury? While understanding and sympathetic to the concerns of many of my fellow historians, it seems that many of the recent critiques of quantification have fallen wide of the mark. My concern is not that of Peter Loewenberg who charged that quantitative methods are ". . . ego syntonic for personalities who need emotional defenses against experience."² Neither do I think that my concerns are defined by C. Vann Woodward, one of our leading American intellectual historians, who observed:

. . . traditional historians have reacted defensively and belligerently [when] confronted with equations they cannot read, with techniques they cannot understand, [and] with copious data beyond their comprehension. They see their authority challenged, their humanistic values threatened, their canons of criticism ridiculed, and their cherished classics derided as "soft," impressionistic, and unscientific. It is not surprising that some of them have overreacted.³

Although I am an historian proud to have been trained in what has variously been described as subjective, humanist, or qualitative methods of historical research, I have been eager, nevertheless, to integrate quantitative methods in a more meaningful fashion into my research methodology. In developing the analysis which follows I am indebted to some outstanding statisticians at my own

university for discussions which helped me to translate my initial historical reactions into more formalized statistical terminology.⁴

My disagreement over Professor Rury's approach is not due to any reservations over his use of quantification, or even the use of any particular statistical technique, but rather, in the broadest sense, over the whole nature of conceptualization and data development in social history. In order to put Professor Rury's paper in perspective, I would like to contrast his own use of quantification with the kind of quantitative history being produced by a group widely referred to as "the new social historians." The new social history seeks to illuminate the day-to-day experiences of ordinary people in past historical periods. It concentrates on the detailed, intensive study of the lives of particular individuals and groups. For example, the Philadelphia Social History Project has painstakingly developed huge computer data sets of information about the family, school, and work patterns for hundreds of thousands of individual lives in late nineteenth and early twentieth-century Philadelphia.⁵ They have divided the city of Philadelphia into wards and gone house-by-house asking such questions as: What was the father's occupation and income? . . . the mother's occupation and income? What was their religion? . . . ethnic background? . . . political affiliations? What were the dimensions of the immigrant family life cycle? Did they send all/some/none of their children into the labor force? Did they take in boarders and lodgers? Did the family own or rent their own home? What was the influence of mass systems of transportation? Where did the children go to school? . . . for how long? . . . with what success? In addition, they have been able to amplify their statistical picture with information on family size and composition, births and deaths, infant mortality, ages at marriage, employment history, school attendance, etc. By combining the statistical pictures of many families, these new social historians can then

can then obtain aggregate statistics for the community, with proper sensitivity of course, to sampling problems. In his now classic article, "A Systematic Social History," Professor Samuel P. Hays has brilliantly delineated the proper categories and boundaries for much of what we now refer to as "the new social history." He writes:

Social history focuses on society and society concerns the relationships among men. The key concept is relationships. Social history is concerned with human interaction, no matter what its manifest topical content, whether economic, political, religious, or intellectual.⁶

Since the task of historically reconstructing and developing statistical techniques to examine these micro-level relationship data sets is such an enormous computer project, one is certainly tempted to see what can be done with the more easily available national level federal census data on American cities. While Professor Rury honestly admits in his Introduction that ". . . the advantage of analyzing a national data-base such as this is a matter of perspective" he proceeds convinced that such a national data base does indeed contain the explanatory power to identify ". . . the broad contours of change in women's education and women's work at the turn of the century, and helps us to see the ways in which they are related" (p. 3). Let us evaluate the evidence.

First, it is important to understand that the data generated by Professor Rury's model explains how cities function, not individuals or groups. On some level, Professor Rury certainly knows this, but he does not make this sufficiently clear in his paper. This is particularly true in his concluding remarks where he would have us believe that his data set has something significant to say about female teenage motivation patterns during this period. Let me explain my concern with a corollary example. If I were to select sixty American high schools and collect data on the extent of drug use and absenteeism among students, the only information that I have is about high schools--i.e., that in those high schools there was or was not a correlation between drug use and

absenteeism. I can not, however, conclude from my survey that the students who used drugs were also the students who were absent. In other words, the information set is about schools and not about students. In the same way, Professor Rury's data set is about the structure and functioning of American cities and not about the individuals in these cities. And yet time and time again, Professor Rury attempts to draw out isolated implications about the women from his data when, in fact, his level of generalization is American cities. This tendency in Rury's work--that is, collection of data on one level and applying it to explain data on another level--statisticians refer to as "the ecological fallacy."

In sum, the difficulty with Rury's statistical model is that it is a sociological model, and not an historical model. This is not to say, however, that there is no historical interest in the statistical information that Rury has collected. The question is one of perspective and emphasis--and how to use such sociological data in informing and enriching our historical models. As Samuel Hays has cogently argued, the proper base and starting point for the development of quantitative models in social history must be on the level of our lived relationships. And it is only within the context of these new micro-level social history quantitative models that the excellent macro-level sociological national data base statistics of Professor Rury take on historical significance and meaning. In principle, micro-history, as represented by the Philadelphia Social History Project, and macro-sociology, as represented by Rury's paper, should be complementary ways of understanding the same social reality. In practice, however, historians have yet to integrate the two approaches.

Finally, in reviewing the statistical procedures used in this study, I was confronted with two related shortcomings in the research: first, the problem of how Rury built his statistical model; and secondly, the issue of the historical

interpretation of his Beta coefficients. First, what was the procedure by which he included and/or excluded his dependent variables? Statisticians tell me that there are three or four statistical models that researchers can use in developing their own unique package of statistical variables. Professor Rury gives no indication as to the process by which he arrived at his final set of variables. In building his model, did he eliminate some variables as statistically insignificant? It appears from Professor Rury's paper that he merely chose those variables that he was fortunate enough to find data on in the census books. Certainly this is no way to build a statistical model. But the fact is, we have just not been informed on Rury's statistical model-building procedures. This is even more puzzling in light of the fact that he has ignored some of the most crucial variables that have shaped the research of much of the previous quantitative work in the field--father's occupation, relative inexpensiveness of female labor, sexual occupational segregation, unavailability of traditionally preferred labor (i.e., male).⁷ Secondly, there is the problem of his Beta coefficients. Are his seemingly etched-in-stone Beta coefficients really as conclusive as he makes them appear? One statistician I spoke to pulled statistics book after statistics book off the shelf to help defend her view that Beta coefficients are extremely variable, especially when confounded with the problem of multicollinearity. She further pointed out that the difference in the Beta coefficients could have been explained by the order in which one entered the variables into the model. At best, she warned, they could only be tentatively interpreted in light of other, more concrete historical evidence from the period. In any event, they have none of the deterministic qualities that Rury bestows upon them in this analysis.

In conclusion, it is hardly the purpose of my response to suggest an alternative research program for American social historians. I intend only to raise

some of the hard and difficult questions that must be posed by historians, sociologists, and statisticians alike as we grope to define the boundaries and the future potential of many of the new methodologies. Hopefully, Bridenbaugh's cry "that Bitch-goddess QUANTIFICATION!" will sound increasingly anachronistic as we learn to integrate its unquestioned potential in more meaningful ways into our historical narratives.

NOTES

*Note to the reader: Professor Rury's revised paper is considerably shorter and has been changed in significant ways from the original paper. I have decided not to amend my original response as it does address clearly the shortcomings in the original paper. The reader is encouraged to contact Professor Rury for a copy of the original 60-page paper with the accompanying 16 pages of statistical tables.

¹Carl Bridenbaugh, "The Great Mutation," American Historical Review 68 (January 1963):326.

²Peter Loewenberg, "The Psychohistorical Origins of the Nazi Youth Cohort," American Historical Review 76 (December 1971):1475.

³C. Vann Woodward, "The Jolly Institution," New York Review of Books 21 (2 May 1974):3; cited in Richard Beringer, Historical Analysis (New York: John Wiley and Sons, 1978), p. 194.

⁴In writing this paper, I have profited from the discussions I have had with some outstanding colleagues at West Virginia University. In particular, I am grateful for the help of: Dr. Shirley Dowdy, Associate Professor of Statistics; Dr. Frederick Zeller and Dr. Wil Smith, Office of Applied Research, Evaluation, and Planning; Dr. Judith Stitzel, Women's Studies Center; and Dr. Carol Henry, Mathematics Education Program, West Virginia University. I am also appreciative to comments by Dr. Peter Sola of Howard University to early drafts of this response.

⁵Theodore Hershberg, ed., Philadelphia: Work, Space, Family, and Group Experience in the Nineteenth Century, (New York, 1981); David Hogan, "Making It in America," in Work, Youth, and Schooling, eds. H. Kantor and D. Tyack (Palo Alto, California, 1982); Michael Katz and David Hogan, "Schools, Work and Family

Life Social History," in Historical Inquiry in Education (New York, 1982); David Hogan, "Education and Class Formation: The Peculiarities of Americans," in Education and Social Reproduction, ed. Michael Apple (London, 1981).

⁶Samuel P. Hays, "A Systematic Social History," in Billias and Grob, American History (1971), pp. 115-66.

⁷Cynthia Epstein, Woman's Place: Options and Limits in Professional Careers (Berkeley: University of California Press, 1970); Randall Collins, "A Conflict Theory of Sexual Stratification," Social Problems 19 (1971-72):3-21; Joan Acker, "Women and Social Stratification: A Case of Intellectual Sexism," American Journal of Sociology 78:936-45; Janet M. Hooks, "Women's Occupations Through Seven Decades," Women's Bureau Bulletin No. 218 (1947); Valarie Oppenheimer, The Female Labor Force in the United States (Berkeley: University of California Press, 1970); Joan Huber, ed., Changing Women in a Changing Society (Chicago: University of Chicago Press, 1973); Donald J. Treiman and Kermit Terrell, "Women, Work, and Wages--Trends in the Female Occupational Structure," in Social Indicator Models, eds. Kenneth C. Land and Seymour Spilerman (New York: Russell Sage Foundation, 1975).